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Chapter 6

Summary and recommendations

To be able to know which development projects work, rigorous research is needed, even in the case of carefully collected data. Organizing data collection in a wrong way can make it impossible to correctly estimate the outcome of interest. This thesis illustrates the importance of organizing research and data collection carefully in the case of health care interventions in an African setting.

After the introductory first chapter, the second chapter is a socioeconomic impact evaluation of the Kwara State Health Insurance (KSHI) program in rural Nigeria, which includes health facility upgrades. Since a randomized controlled trial—the golden standard for impact evaluations—was infeasible from the point of view of the insurer, a difference-in-differences design was followed to control for self-selection into insurance: individual and household level data were collected not only two and four years after the start of the intervention (follow-up data), but before its start as well (baseline data), in a population that received access to the health insurance (treatment group) as well as in a similar population without access to the insurance for the duration of the research (control group). Difference-in-differences relies on the assumption that the two groups would follow similar trends over time, in absence of the insurance. Comparing the two groups over time, the true impact of the intervention can be surmised. By additionally collecting data on changes and shocks that occurred in the communities between the baseline and follow-up, time variant differences could be controlled for as well. After two and four years of the intervention, (modern) health care use in the treatment group had highly increased, which could be attributed to the health insurance program. Out of pocket health expenditures decreased after two years, but were back to original levels two years later.

Chapter three is a socioeconomic impact evaluation of the KNCU Health Plan in rural Tanzania. Just as the KSHI of the second chapter, the KNCU Health plan is a health insurance, coupled with health facility upgrades. The research design and data collection method were similar as well: difference-in-differences, while also collecting data on community level changes over time. However, there are some big differences between the two health insurances and populations. The biggest ones are that the KNCU Health Plan was a household insurance available to small scale coffee farmers of a coffee cooperative (KNCU) and was much less subsidized than the KSHI, which was offered at the individual level to everyone in the catchment area of the participating health facilities, and was heavily subsidized (up to 93%). While almost no-one had health insurance in the KSHI population at the time of the baseline survey and (modern) health care use was low, already 11% of the KNCU population had health insurance at baseline, and many were using (modern) health care. Two years after the baseline survey it was found that the impact of the KNCU Health Plan was in a similar direction as the KSHI impact: positive impact on (modern) health care utilization and negative impact on out of pocket health expenditures.

The fourth chapter is about how biomedical measurements in a survey can change people's behavior and can confound impact estimates (the 'test effect'). It builds on the same data

used in chapter three. At baseline blood pressure was measured in the adult survey sample, before the offer of insurance. The interviewers revealed the results to the surveyed individuals, for ethical reasons. A measurement of high blood pressure can entice people to take up health insurance to be able to get free health care for hypertension, especially in the insurance treatment group where the KNCU Health Plan was offered shortly after. If this is the case, then it will inflate the impact results of the impact evaluation. If care is not taken it will be impossible to separate the impact of the health insurance intervention from the impact of the biomedical measurements during the survey. Thus, the survey itself, by revealing previously unknown information to surveyed individuals, confounds the health insurance impact results. One way to separate the two effects is by randomly excluding people from getting the biomedical measurements during the baseline survey—the strategy adopted in this study—and compare the impact between the group that received the measurements and those who didn't. The results are that people who had a high blood pressure measurement do seek more health care afterward, but no difference was observed between the control and treatment group in this respect, concluding that the health insurance impact results need not be corrected for the biomedical measurements in this particular case.

The topic of chapter five is biomedical measurements as well, but in this case HIV tests. To be able to allocate the right amount of funds to HIV care and prevention it is important to correctly estimate the number of people who are HIV positive (HIV prevalence). The best way to estimate this is to administer an HIV test on a random sample of a population. However, not all people consent to the test. If the part of the population that refuses the test has a different HIV status from those who do participate, the HIV prevalence estimate can be inaccurate. By assigning interviewers randomly it is possible to circumvent this problem and estimate the prevalence correctly, without knowing the HIV status in the group that refused the test. This chapter uses data from Windhoek, Namibia, where interviewers were randomly assigned to households. Some are better at convincing people to take the test and others are worse. By exploiting the randomness of the assignment and the difference in interviewer quality, the HIV prevalence estimate could be adjusted. In this way a Heckman-type selection model with interviewer IDs as instruments could be used to estimate the HIV prevalence correctly, while calculating the standard error of the estimate using the delta method. While no bias in the naive HIV prevalence estimate was found in the total population, it was shown that it was biased downward in the poor and young subgroups.

Several recommendations on how to organize research and data collection arise from the chapters of this thesis. Firstly, if a randomized controlled trial is infeasible when planning an impact evaluation, a difference-in-differences design is recommended, while also collecting data on changes over time to be able to control for time variant differences between the treatment and control group. Secondly, to control for the test effect when collecting sensitive biomedical data—results of which are shared with the person receiving the test—during a baseline survey of an impact evaluation, it is prudent to randomly exclude a subsample from receiving the measurements. In this way the researcher is able to check at the time of the follow-up survey how much (if any) of the program impact is due to the test effect and how much can truly be attributed to the program that is being evaluated. Finally, when measuring the prevalence of an illness, one way to control for refusal bias is to randomly assign interviewers to the surveyed individuals. Using a Heckman-type selection model with the interviewer IDs as instruments and the delta method to calculate standard errors, the researcher can arrive at the correct prevalence estimate.